

Interactive comment on “Organics in the Northeastern Pacific and their impacts on aerosol hygroscopicity in the subsaturated and supersaturated regimes” by K. K. Crahan et al.

P. Chuang (Referee)

pchuang@es.ucsc.edu

Received and published: 19 July 2006

Comments on Crahan et al. “Organics in the Northeastern Pacific regimes”

Reviewer: Patrick Chuang, UC Santa Cruz

Overall:

The paper addresses a topic that has been much-discussed in the recent literature. My overall impression of the paper is that the data is of good quality, and they have some nice cases to look at, but I feel like their analysis could be significantly improved. In particular, it often seems as if the authors end their analysis right when things start

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to get interesting. Also, I did not understand the CCN closure analysis, so I think that needs a good re-write. I believe that there's plenty of good material here, but more work is needed to bring it all out.

Major comments:

Section 3.1 Chemical Speciation

I don't believe that the authors can pick and choose which correlations to look at. There are a number of significant correlations in Table 3, and if you draw the line for significance at some value, say 0.5, then ALL values that reach that threshold should be explained. I must admit that I find the results a bit puzzling, and I'm not sure how to resolve all the correlations, but I think once a coherent picture is put together, it will be much more satisfying.

What is IOM? I don't think it's defined anywhere.

Are the correlation coefficients R or R^2 ?

Section 3.2 Subsaturation

UWPH is not defined or explained at all. It would be nice to show a plot with the data and the fits for k and γ for the periods that are most relevant to this work.

4221, Line ~20: I have a very difficult time believing that the 7% of IPOM observed is, by itself, responsible for the high negative correlations with γ . My guess is that it's more likely that it represents a compositional type that is less hygroscopic. Note that this argument for IPOM is also inconsistent with your argument for ammonium sulfate. Each exhibits strong negative correlations, yet you seem to come to different conclusions. Further, your argument about sulfate is only valid if γ for pure amm sulfate is LOWER than that observed in your data, but you haven't demonstrated if this is true. What is this value? Overall, this part seems to me to need some improvement in order to be convincing.

4222, line 5: do the filters really represent exactly the periods C1 to C3?

4222, line 11: it's unclear where the twelve points come from.

4222, eq. 7: Seems like you need some sort of sum across species.

4224: Is the chemical composition size fractionated in any way? Or are particles of different sizes all represented by the same composition? What might be the sensitivity of these results to whatever assumption you use here?

4225, line ~10: Maybe this is oversimplifying the argument, but the authors' primary claim seems to be that "Models don't work. Therefore organics are responsible." This doesn't seem sufficiently satisfying to me. Is there some way that this argument can be made much stronger? This connects to the next comment as well.

4225, line ~15: The title of the paper suggests that these issues should be the heart of the paper, but instead the analysis in this section stops right here. I acknowledge that it gets difficult right about here as well, but I believe it would make the paper a much improved and important contribution if this is pursued!

Section 3.3 Supersaturation

4225, line 19: If water activity is depressed, then critical supersaturation decreases, not increases. When referring to a change in critical supersat, what is it relative to?

Line 24: Ellison et al. was far from the first to suggest this. Also, micelles aren't really necessary for this process. See Barnes (1986) for a review of lots of older literature. You can find this reference in Chuang (2003), wherein there might be other relevant references as well.

4226, line 12: "instrument noise" - do you really mean noise? Or is it just variability that you're observing?

Line 17: It would be nice if we could see the actual data, to evaluate how well the fitted curves represent the data.

Line 16-18: These supersaturations don't really match well with the PCASP sizes, at least for simple salts. Isn't this a problem?

Line 19 and on through 4227: I read this section about 5 times and for the life of me couldn't really understand how you were doing this CCN closure. So much so that I couldn't really evaluate anything after this because it all depends on the way the closure was done. This needs to be expressed much more clearly!

Figures:

Figure 4: Is this really closure? I don't think so, since mass isn't measured, i.e. you're not comparing some derived quantity against a measured quantity.

Figure 7: Top two plots: aren't the lowest DGF values shown as asterixes meant to be "x"s instead?

Looking at these plots, what I see is that each model yields some DGF that is nearly constant. Kohler is 1.6 to 1.7, Svenningsson is 1.5 to 1.6. But observed values for C1 and C3 get down to 1.4. How should this be interpreted?

Figure 8: How about representing the data as points instead of a line? Then we can see how well the fit works. And why the 5 orders of magnitude on the x-axis?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4213, 2006.

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